

ART. III.—HEMIPLEGIA—WITH LESION ON THE SAME SIDE OF THE BRAIN.

BY WALTER HAY, M. D.

IN the Review of the Transactions of the American Neurological Association, for 1875, in the April number of the Journal of Nervous and Mental Diseases, occurs the following: "Dr. Hay's case of hemiplegia, from cerebral lesion on the corresponding side of the brain, is one of a class of observations, that is always open to question, and, in the opinion of a such a clinician as Charcôt, much more than doubtful."

This very ambiguous phraseology admits several constructions, and may be assumed to question the veracity of the statement, the accuracy of the observation, or the inference legitimately deducible therefrom.

The exhibition of the specimen to the Association, precludes the first construction, were such even within the thought of the reviewer, which the writer will not do him the discredit to suspect; an insinuation, so personal, would derogate too much from the dignity of the Journal of Nervous and Mental Diseases.

This circumstance, too, disposes, in part, at least, of the second construction, inasmuch as accuracy of observation was susceptible of confirmation, or refutation, by any member of the Association,—that is, of observation of gross changes, the report expressly disclaiming any comprehension of microscopic conditions in the case.

That the recognition and consideration of such changes, was unnecessary to a full and complete explanation of the lesion, and its physiological relations, careful analysis of the report will demonstrate.

Under the third construction suggested, the language of the reviewer is less unintelligible, for, while the existence, or non-

existence, of such facts is solely a question of veracity, or of accuracy of observation, the conclusion, deducible from their comparison, are always subjects of discussion, criticism, and doubt.

Before considering the conclusions, a brief review of the facts from which they were deduced, is in order.

The first of these is the condition of the subject, previous to the reception of the injury; a healthy, robust woman, without evidence of mental or physical disease; the next, the condition of the same subject immediately after the reception of the injury, as noted by Dr. Owens, upon her admission to St. Luke's Hospital, Nov. 2, *i. e.*, "hemiplegic on the left side, and unconscious."

To an ordinary mind, not preoccupied with a theory, it would seem absurd to attempt to assign any other cause for the hemiplegia and unconsciousness than the injury, to which their relation as effects was so direct and immediate.

This point admitted, and the ultimate cause of the hemiplegia thereby determined, it remains to indicate the relations of intermediate or proximate phenomena.

Of these, the next in order, observed and noted by Dr. Owens, is the manifestation of evidences of insanity, Dec. 19, forty-seven days after the reception of the injury.

That the pathological changes expressed by this insanity were epiphenomena, and altogether secondary to the traumatic cause which induced the hemiplegia, is indicated, by their gradual development, by the length of time occupied therein—allowance being made for the insensibility, which would mask their appearance—and by the manifestation of some, although feeble, intelligence, in the interval.

It is noted on Dec. 13, that, "By opening the eyes with the fingers, and at the same time using strong impressions to attract the attention of the patient, she could be made to look in directions indicated." In this there is exhibited a mental condition differing essentially in its character from the state of complete incoherence, which prevailed subsequently, and which was permanent. Although the faculties of expression were almost entirely in abeyance, they indicated as far as they could, intelligence; which they failed to do a few days later.

From the evidence thus far adduced, it is demonstrated, that the insanity was the result of pathological changes in the brain, developed gradually, after the establishment of the hemiplegia, which supervened immediately upon the reception of the injury.

The order of occurrence of the phenomena, determined by the evidence, was, first, a condition of mental and physical health, in which the existence of any brain lesion was inadmissible; second, the reception of an injury; third, a hemiplegia; fourth, pathological changes in the brain, and fifth, insanity.

It is impossible that the hemiplegia could have resulted from these pathological changes, which were the direct causes of the insanity, in as much as it preceded them in the order of its occurrence. Effects can not precede their causes. So far, then, as their relation to the hemiplegia is concerned, these pathological changes which resulted in the insanity may be excluded from consideration altogether. The injury and its immediate consequences must have been the sole causes of the hemiplegia.

What were these immediate consequences, is the question. That profound unconsciousness supervening immediately upon an injury, accompanied by hemiplegia, and continuing through many days, should justify the diagnosis "meningeal hæmorrhage," will scarcely be denied, and when superadded to this condition, another symptom, not yet indicated, aphasia, supervenes, a reasonable conclusion concerning the locality of the hæmorrhage, is justifiable.

The disregard of this obvious relation of symptoms, the inference deducible therefrom, and the diagnosis expressed in the report, "clot on the right side," only illustrates the influence of prescript and habit of thought upon the mind of the writer, not yet daring to doubt the "traditions of the elders."

The erroneous diagnosis is, of course, contradicted by the autopsy, which reveals meningeal clot on the left side, with the evidences of meningitis surrounding it on every hand.

Now, under ordinary circumstances, the discovery of a clot in the brain, in a case of hemiplegia, and of the location of that clot within the membranes, when profound unconscious-

ness continuing many days, accompanied that condition, would be regarded, as a matter of course, in entire harmony with our pre-conceived ideas. But, when the clot is found in a position differing from that in which, previous observation had suggested its location, what is done? Is the fact accepted as a new datum, for the basis of a more comprehensive induction, which might result in the establishment of a new principle? By no means. A theory has been presented, brilliant, fascinating, plausible, the fact is applied thereto, and found discordant therewith, therefore, what? Is the theory discredited? Not at all; the fact is rejected. A square inch of foreign matter in the brain must be assumed to be innocuous, the correlation is denied, the observation, "is open to question," and, "more than doubtful."

The clot caused nothing, or something; if nothing, then it must be admitted that a foreign substance, an inch or more in length, may rest upon the surface of the brain, compressing it, with impunity—which contradicts all clinical experience; if something, what? Either the hemiplegia, or the unconsciousness, for these were its only coincidents.

But the unconsciousness disappeared, so did not the clot, while the hemiplegia was permanent, as was the clot.

It is assumed by the doubters, that the cause of the hemiplegia must have been on the opposite (or right side); if this be so, then the clot was without result and harmless, which is absurd.

An obvious, sufficient, and common cause must be ignored in the search for a recondite mystery; a gross mechanical lesion, involving a large area of brain, could not have entailed consequences, which must have resulted from microscopical changes elsewhere, in such an anatomical *terra incognita* as the brain.

Concerning the supposed presence of lesion in the right side of the brain, it may be asked, what was its origin and its symptoms? Did it antedate, coincide with, or succeed the injury? If the first, it was altogether latent, as there were positively no symptoms; if the second, and indicated by the hemiplegia, then the clot on the left side must have been without result and harmless, a *reductio ad absurdum*.

The impression of the reviewer, concerning the opinions of M. Charcot upon the subject of direct hemiplegia, is evidently an inference deduced from the expressions of that physiologist, in the discussion with M. Brown-Sequard, at the meeting of the Société de Biologie, Dec. 18, 1875; and only an inference, as there is no evidence to show that M. Charcot's cases included any resulting from traumatic causes, and hence this case may reasonably be excluded from those, which he considered "more than doubtful."

But leaving the "eminent clinician" to the tender mercies of that other "eminent clinician," Brown-Sequard—who cites no less than two hundred cases of hemiplegia, with cerebral lesion on the same side—the writer must insist upon the accuracy of the observation, the truth of the statement, and the legitimacy of the deduction.

The review is evidently written by one, who, captivated by the very brilliant, and also by the very hasty generalizations of Hitzig, Ferrier, and others, concerning the localization of functional centres in the brain, has failed to subject the same to the test of strict logical analysis; had he done so, he would, perhaps, have discovered, that however plausible, they are not sustained by conclusive evidence, and the doctrine yet be endorsed *non probatur*.

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[The idea intended to be conveyed by the passage in the review of the Transactions of the American Neurological Association, in our April number, to which Dr. Hay takes exception, was that cases of the kind reported by him being so exceptional and contrary to usual clinical experience, were always open to doubt, either as to the accuracy or completeness of the observation—not necessarily to the denial of either, though M. Charcot does not hesitate to reject them altogether. It was not intended to convey the impression, however the wording may appear, that Dr. Hay's observation was necessarily lacking in either respect, but that it belonged to a class that were generally doubtful. The unfortunate phraseology that renders possible even the suggestion of a doubt of the re-

porter's veracity is greatly to be regretted, as nothing was farther from the intention of the reviewer.

There is, we say, no absolute necessity for questioning the accuracy or completeness of Dr. Hay's report. There is, however, we hold, ample reason for doubting the correctness of his deductions, and the great importance of this observation as contradicting the received views of cerebral pathology. It will be remembered, perhaps, that M. Charcot, in his discussion with M. Brown-Sequard in the *Soc. de Biologie*, (published in our April number), declared that cases of tumors were unsuitable for the study of cerebral localizations, since they had no definite symptoms, and merely displaced the tissues without destroying them. We know of no reason why the same condition of things might not occur in a meningeal hemorrhage, such as the one in the case reported by Dr. Hay, which was said to have indented the surface of the "frontal convolution of the left side," and to have been accompanied "with fibrinous effusion extending over a considerable portion of the hemispheres on both sides." And if this is possible in any case of meningeal hemorrhage, it vitiates every such observation for deciding the question of the possibility of direct hemiplegia.

The point made by Dr. Hay as to the opinion of M. Charcot, does not seem valid, since that pathologist did not exclude traumatic cases from his general condemnation, and did not even make an exception of the experimental lesions adduced by M. Brown-Sequard. And we can ourselves testify, that many of the cases on which M. Brown-Sequard supports his views of cerebral physiology and pathology, are not reported with the accuracy required to render them of any conclusive value in deciding modern pathological theories, the cases reported by Gintrac, for example, which are altogether valueless for any critical purposes. It must be remembered, moreover, that Charcot has utilized greater opportunities in nervous pathology than perhaps any other living man, and that after a careful revision of the whole matter, he has reiterated his opinions still more positively. He says, in a recent lecture alluding to the discussion with M. Brown-Sequard, "In the presence of an op-

position from so high a quarter, it became my duty to submit the question in dispute to a complete revision, to ascertain whether I had actually fallen into error, and led others with me. In case I had found myself wrong, I would have been here to-day to resolutely confess my mistake, and make honorable amends; but this has not been the case. My new studies, undertaken in the same direction as those stated to my hearers the past year, have only strengthened my previous convictions."

When we consider the vast opportunities enjoyed by M. Charcot for the study of this question at the Salpêtrière, where, as he states, there are never less than four hundred hemiplegics, and the manner in which he has utilized his great clinical experience, it seems no disparagement to say of his opponent, M. Brown-Sequard, that, as an "eminent clinician," he is not to be compared with M. Charcot. And when we remember the support afforded the latter in the discussion by M. Luys, one of the greatest living authorities on cerebral anatomy and physiology, it is not difficult to see on which side of the question lies the weight of authority.

Although, as has already been said, there is no absolute necessity for supposing that there was any other damage in the case of Dr. Hay, than the one reported, it being possibly a mere pressure lesion, one of a class the effects of which are generally indefinite and indecisive as to pathological questions, yet the admitted lack of a microscopic examination deprives it of that scientific completeness, that should be demanded of every observation claiming to overthrow established theories. Cases have been published, in which extensive cerebral disorganization has existed, causing serious functional derangements, altogether imperceptible upon merely macroscopic observation. Such a one was reported by M. Pitre in the discussion in the Société de Biologie. Though there may have been no such lesion in the case in question, the failure to exclude it renders the observation inconclusive, and of far less value than Dr. Hay seems inclined to esteem it. The possible consequences of a traumatism are so numerous, and the possible results of a meningeal hemorrhage so various, that no positive

scientific conclusion can be drawn from the data of this case.

Dr. Hay says: "An obvious, sufficient, and common cause must be ignored in the search for a recondite mystery; a gross, mechanical lesion, involving a large area of brain, could not have entailed consequences, which must have resulted from microscopical changes elsewhere, in such a *terra incognita* as the brain." To this we would say, that we do not ignore the obvious lesion, nor, at the same time, the fallacy which exists in being contented to connect such a paralysis with such a lesion, without further investigation, or that would term such a cause "sufficient." Facts, both experimental and clinical, almost *ad infinitum*, go to show that cortical lesions are not necessarily, or even generally connected with permanent paralysis; in fact such a lasting hemiplegia, as in the case in question, is fair ground for suspicion of damage elsewhere. That this damage elsewhere is a "recondite mystery," in this case, is, we think, owing to Dr. Hay's failure to utilize his opportunities for the exhaustive examination that alone could have furnished the positive and negative evidence required to justify his conclusions. The brain is always a pathological *terra incognita*, until it is thoroughly examined.

The trouble with this, and all other similar cases, is not so much what is said, as what is left unsaid. The author seems to have failed to recognize the want of necessary connections between his conclusions and the data of his observation, and to have ignored the wilderness of possibilities that lies between them. One serious need of the times, is closer and more thorough observations, as well as the logical ability to correctly deduce conclusions from them.

We have given so much space to remarks on this subject, not on any personal grounds, nor so much to relieve ourselves from the imputation of having sacrificed a fact for the sake of a theory, as for the purpose of re-stating our opinion on what appears to us to be a very important pathological question, in regard to which we seem to be at variance with the author of the above article.—Eds.]